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CONTRIBUTIONS TO THE LIFE HISTORIES OF PLANTS, NO. XII.

BY THOMAS MEEHAN.

It will be seen by the dates and references in some of the following chapters, that the papers were written long ago. The facts noted have been confirmed by observations made in subsequent years. Some of them have been read at the meetings of the Botanical Section of the Academy, and, though requested for publication elsewhere, have been held so as to appear in this series of "Contributions."

Having been written at various times, when the subjects of the sketches were fresh in the mind, there may be some repetition of propositions. This would not have been the case had the papers been prepared continuously.

THE FECUNDITY OF HELIOPHYTUM INDICUM.

Heliophytum Indicum, the *Heliotropium Indicum* of the older botanists, has found its way over all the tropical and subtropical portions of the earth. It is at home in Asia, Africa and America, and if it once gets a chance seed into the soil of Europe, will no doubt as easily maintain its hold as other free-seeding typical weeds have done. In some unknown way a few plants appeared in 1894 in my garden, and have afforded me an interesting study.

Its capacity for seed production is enormous. The cyme-branches that have flowered and have, or will have, perfect seed, represent, August 28th, a line of 1,224 inches. There are twenty seed vessels, that is to say forty seeds to the inch, making a total of 48,960 seeds. The cymes are still vigorously unfolding and flowering, and will probably double these figures, but in uncertainties it is best to be on the safe side; so, allowing but one-third more, we have a length of fruiting rachis of 1,632 inches, and a total seed production of 65,280.

All this has proceeded from a plant that was itself but a seed three months before! The total length of stem and branches supporting these seed-bearing cymes, is but 396 inches. The plant is true to the classical story of Clytie and Phoebus which gave the original genus *Heliotropium* its name. It does not open a flower until the sun has reached the summer solstice. When the sun ceases to woo it, the flower opens, only to find its beloved going away. Less than three months of flowering will, therefore, have been occupied in this enormous seed production.

The facts here detailed have an important bearing on two points maintained by me, in connection with the life-history of plants:—

I have recorded numerous observations in the *Proceedings* of the Academy, commencing with 1866, showing that the growth-energy of plants is rhythmic, dependent on the power of the plant, or the parts thereof, to convert nutrition into the growth-force, and that the various forms which plants present are the result of varying phases of life-energy, in most cases of no physiological value, and with which environment has little to do. The evidence furnished by *Heliophytum*, though of a negative character, is surely strong. Through the long ages the plant has been established over a vast area, and consequently subjected to many varying and varied conditions of environment; it has continued as a compact genus or section distinct from *Heliotropium*, without any material change that would warrant a modern botanist in making new species of it.

Again it has been maintained by me that as environment can have no important influence on changes of form, the free and untrammelled production of seed would be of far more importance in a supposed "struggle for life" than any power of adaptation could be that depended more on an occasional cross for its increased energy. Dean Swift's satire in which the Lilliputians, by the mere force of numbers, are made to overcome the giant Brobdingnagians, cannot be supported in every case by the histories of plants, but when it comes to a question of distribution, numbers surely are the more likely to hold the field.

I think I may claim the credit of advancing the further proposition that a free production of seed may always be taken as an *a priori* indication of self-fertilization. In cleistogamic flowers the certainty of seed-bearing is well known. With rare exceptions the huge natural order of Compositæ are self-fertilizers, and they have managed to embrace within themselves about one-tenth of the whole vegetable kingdom. Where the wind or an insect is the agent in fertilization, the agent does not always come along. On plants dependent on this outside assistance, numerous flowers fail to seed. No plant so dependent ever perfects all its seeds; in many cases utter failure follows. In this remarkable plant there is no indication that a single flower failed to mature seed. It must certainly be held remarkable that in a single plant, bearing in round numbers over 30,000 flowers, every one should bear two seeds.

It has been contended that though plants may generally self-fertilize when the agents for cross-fertilization do not attend, they are so arranged as to cross-fertilize when the agent does appear. As the *Heliophytum* flowers are freely visited at times on my grounds by insects, and especially butterflies, there might be some strength in the point. I can, however, testify by an almost daily observation of my plant through the season, that minute flowers are only visited by insects when others are scarce. Though I have seen them visiting the flowers for several successive days, there are many days when they do not visit them, and none were noticed on the former until the beginning of August. A careful watching of the anthers shows, however, the extreme difficulty of effecting cross-fertilization. The anthers form a cone over the stigma, and the pollen sacs burst almost simultaneously with the unplaiting of the corolla. When the flower is a few hours old the stigma protrudes slightly through the anther-cap, and is visible under a lens through the very small orifice of the corolla-tube. Even admitting that the flower has not fertilized at this early stage, and that the tongue of a butterfly might introduce foreign pollen to it under certain circumstances, it would rarely, in any case, occur. It is well known that all insects soon discover the easiest method of doing their work. In this case there are five openings between the tube of the corolla and the bases of the filaments, offering a wholly unobstructed course to the creature's tongue. It would have to use considerable force to insert its tongue under the anthers pressing down on the stigma. It is inconceivable that the flower can receive any aid to cross-fertilization in this way. But we may grant that a cross-fertilization will result in a plant better fitted for the struggle for life than one self-fertilized, and that a small percentage might become cross-fertilized. The question of numbers again forces itself upon us. How many of the seeds of any plant get a chance to develop to a plant again bearing seeds? How many of the 65,280 seeds of this plant will probably mature; will come to be seed-bearing plants next year? Only a small percentage, in any case, ever do. In this case surely very few will, and of these how many would those resulting from a "chance cross" give?

Facts of this character are common, but this case presents them in such a remarkable degree, as to make it a special one *Heliophytum indicum,* a self-fertilizer and wonderfully productive, has maintained a remarkable homogeneity amidst rare variation in the environmental conditions.

If we accept the thought frequently thrown out in these contributions that form results from the various degrees of rhythmic energy in the plant itself, just as it would in the formation of the frost crystals on a window pane, we can see that environment can not be regarded as a leading agent, but must take a minor place.

THE ORIGIN OF THE FORMS OF FLOWERS.

In my intercourse with intelligent and observing botanists, who frequently do not place their conclusions on record, I find a growing tendency to discredit views, till recently widely prevalent, that external conditions have any more than a feeble influence on the evolution of the forms of flowers. Thought is in the direction that various degrees of internal energy seem rather the chief agents in effecting change.

Listening to some verbal remarks before the Botanical Club of the American Association for the Advancement of Science, at Buffalo, New York, by Mr. David F. Day, I was struck by his point that irregular flowers were usually associated with the curving or twisting of the peduncle, while regular flowers and straight peduncles were usually closely related. I saw this subsequently well illustrated in lilies. In the class to which belong *Lilium Philadelphicum* and *L. Catesbœi,* the upright flowers are perfectly regular; irregularity, in some of the floral parts, characterizing the nodding ones. The nodding peduncles, after flowering, become erect, and in the seed-bearing stage the seed vessels are erect on perfectly straight peduncles in both classes. It is evident from this fact that in the species with drooping flowers, the expansion of the perianth occurred before the uncoiling energy had been exhausted, and during a rhythmic rest. It is further evident that the growth-waves prevailing in the development of the flower varied in intensity in different parts, and that varying forms must necessarily follow from these varying degrees of energy. Unequal pressure by reason of the curve ought to be accountable for this inequality. It is, however, evident that outside agencies could not have had much, if any, influence in the curving which results in irregularities of these lilies. Some excellent illustrations are often seen where an erect flower occasionally occurs on a plant which generally has the pedicels more or less curved. Some Gloxinias and other Gesneriaceous plants will readily recur to the intelligent observer. *Gesneria elongata,* a South American species, popular in garden culture, often has these erect flowers. In this case the flowers are perfectly regular, and of a different character in other respects from the normal ones.

During the past season I was able to add a new illustration to the list in *Pentstemon barbatus.* In a large bed with several hundred flower stems, I collected some twenty erect flowers. In the normal condition, the three lower segments constitute a lip, and are so tightly recurved that they press against the tube ; the upper two are erect, and form an upper lip. But in the exceptional flowers noted, this is all changed. The lobes of the corolla are equal, recurved, and pressed against the tube. But the most remarkable change occurs in the fifth or barren stamen. In the normal form this is so differently constructed from the other four that thoughtful observation has to be given before deciding that it is a stamen at all. In these erect, regular flowers there is not the slightest difference between any of the stamens. The fifth is the exact counterpart of the other four. Each one of the five stamens are alternate with the five regular lobes, as they should be in any well-ordered regular flower. Assuredly if a plant always had flowers like these, and only these flowers, it would not be a *Pentstemon,* but be made to constitute a wholly different genus, if it were not, indeed, referred to another natural order, for a two-lipped and more or less irregular corolla is regarded as a leading characteristic in Scrophulariaceæ.

We may say that nothing but a different degree of growth-energy, accelerating or retarding the spiral development, so that that which should have been left curved was advanced to (or left in) a straight condition, had anything to do with the remarkable change.

And then we may ask if such remarkably distinct forms can be produced on the same plant, and in an exceptional way, what is to prevent the plant from regularly exercising the same force, and thus making the irregular flower the exception ? That this can be done is shown in the case of the upright and nodding lilies already cited, though we have no evidence that a regular and irregular lily ever grew on the same plant, as here produced by a *Pentstemon.* Examples might be found if carefully looked for.

That these vagaries, once brought into existence, have hereditary powers, is too well known to horticulturists to need more than a passing notice. That they are not oftener the parents of a line of new species is probably owing to the fact that of the millions of seeds produced by a single plant, an extremely small percentage ever get the opportunity to grow and again develop to a seed-bearing condition. There would be little chance among so many for these exceptional flowers of *Pentstemon* to perpetuate themselves.

Though it would seem that in this case environment, as it is generally understood, could have had little to do in developing an irregular to a regular flower, one may plead for life-energy as the chief factor in the production of form, and still leave considerable for environment to do. One cannot well retain as erect a position when holding an umbrella against a driving storm, as if he were simply shading himself on a calm summer day; and there must be some opposing elements or adverse circumstances capable of depressing life-energy as a mechanical force, and with this variation in degree, we may reasonably look for a change in form. But granting all this it must be evident that life-energy, dependent on varying phases of nutrition, is the main power in deciding form.

SPINES IN THE CITRUS FAMILY.

The spines which often occur in members of the Aurantiaceæ or *Citrus* family are said to be axillary. We are to understand by this that they are situated between the base of the leaf and the axis or stem ; but they are rather lateral than axillary. Lateral spines are to be considered as of stipular origin, but stipular spines are usually in regular pairs, one on either side of the petiole. A single lateral spine could scarcely be stipular. Pondering over these anomalies with an orange tree before me, it was noted that the spines varied much in size and strength. In many cases there were no attempts to form spines at the nodes. In a few instances where spines were wanting, the axillary buds were pushing into growth, where a small point with an articulation might be noted on the bud scale. In some cases there would be points on two separate bud scales. With this suggestion a careful examination of the spines showed clearly that they were but strongly developed bud scales, or portions of the calycine system. I could trace no articulation as in the young state above noted, though the upper portion soon loses its vitality, while the major part of the spine remains green and active for a long time.

Examining next some plants of *Triphasia trifoliata* which had wintered in the open air, I was pleased to find the leaf origin of these spines confirmed. The articulation, which the leaves of most of the Citrus tribe have near the junction of petiole and lamina, is plainly seen in these spines (Fig. 1). The part above the articulation is completely dead and brown as one might expect the lamina of a leaf to be—the more highly vitalized spine, the metamorphosed petiole, has resisted the frost killing severity of the winter.

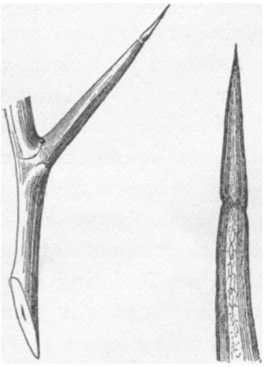

FIG. 1.

We can thus see that the proper classification of the spine in the Citrus tribe is with such as are borne by thistles or similar plants, where the leafy bracts forming the involucres terminate in sharp rigid prolongations, which are not to be classed with spines equally with the sharp apices of *Gleditschia*, *Maclura* and other specifically spiny species.

FLOWERS AND FLOWERING OF LAMIUM PURPUREUM.

One might suppose that a plant so widely spread over the world, and one that intrudes itself so persistently on every one's attention as *Lamium purpureum*, could not possibly have anything written about it that would be new to botanists. But I am inclined to believe that plants have not a uniform behavior in every place, and possibly the behavior of species here may be different from that in the Old World. These considerations make it the more important that the points I have noted in the plants growing on my grounds should be placed on record.

The species is very abundant as a weed on my grounds near Philadelphia. It is the form with the smaller flowers and without the ring of hair below the throat that is described in the typical form of Europe. It was originally introduced into my grounds from Germany. It is probably the form which Willdenow regards as a good species, and describes as *Lamium incisum*. The hair that is found in the throat of *L. purpureum* is absent—there is but a single short tooth instead of two on the lower lip, and the pollen is orange instead of bright scarlet.

Dr. Bromfield notes " anthers with several tufts of stiff hairs or bristles on the face of each cell, and according to Mr. Leighton accompanied by 6–8 small, white, oval, tuberculate bodies at their base, but of which I can find no trace in my specimens, and presume they are, therefore, not constantly present." These tubercles are only on one side of the anther, and are easily overlooked. There are always six of these so far as I have examined them; they are, however, easily overlooked as they are on one side only, as already noted. They are very beautiful as seen under a lens, but I have been unable so far to trace their morphological significance. A small gibbosity on the underside of the tube near the base seems to have been overlooked and may also throw some morphological light on the structure of the flower.

An interesting peculiarity is that soon after the ringent corolla opens it separates from the receptacle, the style disarticulating from the carpels at the same time and falling away with the corolla. In most monopetalous flowers the pistil remains after the corolla fades, the corolla usually falling forward and over it. There is rarely any articulation by which the style separates from the carpels as in this case, and as do the stamens in many flowers of other species.

This early fading of the flower and casting off of the pistil, indicates that the flower may have been fertilized before the opening of the lobes of the corolla. An examination shows that this is really the case. When the flower is fully expanded, the stamens are straight, bearing the anthers under the arched upper lip. The anthers will usually be found destitute of pollen, while the longer lobe of the divided style will be found with an abundance of orange colored pollen at the apex. It will further be noted that from the relative position of the forks of the style and the anthers, the pollen could not readily reach this portion of the style while in the fully expanded condition of the flower.

If we take a matured flower bud, just ready for expansion, and carefully cut away the upper lip, we find no stamens there at that stage. They, as well as the style, are bent downward, grasped and held in place by the incurved lower lip. Examining these unopened flowers in the early morning we find the anther cells have already opened, and the sharp stigmatic point of the lobe of the style in most cases is inserted in one of the anther openings and covered with pollen. Between eight and nine o'clock in the morning the unopened buds unfold. Slowly the incurved stamens and style straighten themselves, and rise to their final positions under the arched upper lip. During this process the stigmatic lobes are brought into contact with the disrupted pollen sacs, and receive an additional supply of fertilizing material, as if nature was taking a double care in this instance that the flower should be self-fertilized. The corolla at this stage seems firmly attached to the receptacle, but very soon afterward it falls at the slightest touch, indicating that fertilization has been perfectly accomplished. An examination of the flowers at this stage will also show that the seeds are wholly mature, and we have to conclude from this examination alone that the fertilization was accomplished in the unopened flower.

CLEISTOGAMY IN UMBELLIFERÆ.

So far as I know no record has been made of cleistogamy in the Umbelliferæ. In 1893 a plant of *Cryptotœnia Canadensis* in my garden indicated cleistogamy, but as the flowering period had advanced considerably, further observation was left for the present season.

Cleistogamy, as usually understood, perfects seeds when the generative organs are enclosed in the calyx only, no attempt at forming a corolla being made. In this strict sense the *Cryptotœnia* would not be cleistogamous. But the plant has two classes of flowers: one in which the stamens and corolla are highly developed, with the gynœcium abortive, the other with a highly developed gynœcium, stamens with comparatively short filaments, but with polliniferous anthers all enveloped in a corolla extremely fugacious, beneath which fertilization is accomplished before opening. The fine anthers are pressed tightly against the stigma, accomplishing fertilization and inducing carpellary growth before the corolla has reached perfection. When this period has been reached it falls at once, carrying along with it the stamens which have already performed their functions. Though differing somewhat from the usual character of cleistogamy, the action is cleistogamic surely.

The corolla in the male flowers is more enduring than in the female flowers; indeed, as a general rule, it is only the male flowers that we notice in examining the plant. It is extremely rare to find a female flower with an expanded corolla, as it falls very soon after opening, and it was this seeming absence of corollas in the first instance, yet with an abundance of fruit-bearing pedicels, that led to the suspicion of cleistogamy.

The numerous male flowers with even more perfect stamens that are more abundantly pollen-bearing than those in the seed-bearing flowers, with absolutely no function to perform, present an anomaly, and yet it has a counterpart in the petal-bearing flowers of many cleistogamic species, which I have found rarely seed-producing, though it is customary to refer this class of flowers, in such cases, to an effort on the part of the species to secure an occasional cross. It would rather seem that the true position of cleistogamy in the economy of nature is not yet well understood.

RHYTHMIC GROWTH IN PLANTS.

Though the principle that plant-growth is not continuous but rhythmic must have been long ago observed, I am not aware that any special importance has been attached to it, or of any detailed observations as to the time and manner of the rests and advances until my paper on the Compass Plant, *Silphium laciniatum*, appeared in the Proceedings of the Academy in 1870. I believe it has been left wholly to me to show that rhythmic growth is an important factor in the evolution of form. It is not even yet recognized as it deserves to be. This consideration renders the recording of additional facts desirable.

Dr. Asa Gray, in the *Synoptical Flora of North America*, referring to the natural order Polemoniaceæ, says, " hypogynous disk generally manifest," and " remarkable among the hypogynous gamopetalous orders for the trimerous pistil, but in two or three species of *Gilia* dimerous." "The corolla is not always perfectly regular, and the five stamens are very commonly unequal in length or insertion."

Of *Phlox* particularly he says, " most species with long filiform style, about equalling or surpassing the corolla tube, but some with short included style, perhaps but dimorphous; but only in *P. subulata* have both forms been found in the same species."

Unless we remember the rhythmic character of growth, we get but a poor idea of the true nature of things by the terms Dr. Gray employs. What do we learn of the disk by merely calling it by this name; and what is a dimerous or a trimerous pistil? Why should the stamens and style be dimorphous, and what is dimorphism in this connection? Before answering these questions, let us follow the development of the flowers in *Phlox*.

Early in the morning of September 1st, all the expanded blossoms of some heads of garden varieties of *Phlox paniculata* were plucked out so as to watch the behavior of the unexpanded flowers. As I believe growth is modified by temperature it may be noted that it was cool for the season—65° F. There was absolutely no growth in the corolla until 9 A. M. Up to this time the corolla was enjoying a rest, but then the lobes began to unfold, occupying about two hours to reach their normal salver shape form. By opening the flowers at various times before 9 A. M. we note that the anthers are not at rest; they are regularly increasing in size. By 9 A. M. they have reached their full development, and the cells burst and discharge the pollen simultaneously with the unfolding of the corolla lobes. By now opening a flower-bud that is a day behind this one in development, we note that while the corolla with its attached stamens was growing, the style was at rest. It evidently starts on its new rhythm of growth the evening before the full opening of the corolla, that is to say when the ultimate length of the latter has been reached, the style starts on its advance. It does not quite reach its ultimate length when the anther cells discharge their pollen, but the appressed stigmas receive and retain the scattered pollen grains. Returning now to the corolla, we find that after full expansion, at about 11 A. M., it continues without change the whole of the next day, withering and falling away on the third. It may be noted, however, that a very light touch causes disarticulation the second day, showing that fertilization has already been accomplished. The lobes of the pistil do not diverge until the second day, but it is evident that fertilization is not dependent on the expansion of the lobes. The pollen tubes act in advance of the expansion of the lobes.

That the morning was rather cool for the season may have been a reason why no insects visited the flowers till noon. Humble bees were the first in the field, rifling the sweets by cutting the tubes of the corolla. The honey bee soon followed, using the slits made by the humble bees, and during the afternoon several species of lepidoptera gathered the nectar by the legitimate entrance.

The nectar is secreted by the " hypogynous disk." It is remarkable, however, that I could find no exudation on the first day of opening. The flow seems to commence on the second day, and is most abundant when the flower is about to wither and long after the fertilization of the flower has been effected. I have already placed this fact on record in connection with *Lonicera* and some other flowers. It is probably a general fact, strangely overlooked in treatises showing the relation of the honeyed secretions to the agency of insects in the cross-fertilization of flowers. So far as the fertilization of *Phlox* is concerned, every fact pointed to self-action. Insects seem to have no agency whatever in the remarkably prolific results. I have before had occasion to remark that abundant fertility in any plant is always a strong indication of self-fertilization. The " dimorphism " referred to might properly be termed polymorphism. Neither the stamens or pistils can be classed distinctly as long or short, but vary indefinitely. In a white variety of *Phlox pyramidalis* I found the style usually about two-thirds the length of the tube. The stamens too, are very variable even on the same plant. At times only one stamen may be seen at the apex of the throat on a second-day flower. At other times there are three, simulating a three-lobed style. There are, however, always to be seen two shorter than the other three. These two lower and shorter are usually near together, but often widely separated, and occasionally one anther will be sterile. The reflecting observer will have no difficulty in referring all these variations to the varying degrees of rhythmic energy. Whether a growth-rhythm flows gently or is arrested suddenly, decides the direction and degree of the rebounding energy, of which the forms of flowers are but the outward show.

Referring now to the hypogynous disk, or nectary as it might be fairly called, an examination when the seed capsule is nearly mature, shows it to be formed of a fine-toothed membrane, the lobes of which are opposite to the lobes of the calyx. Under morphological law we should expect them to be alternate. Separating, however, a nearly mature capsule from the " hypogynous disk," we see by the glass the imprint of five minute scales, alternate with the teeth of the disk; this furnishes the key to the problem. Between the calyx cycle and the corolla cycle, there are two cycles undeveloped, of which the " hypogynous disk " is the only manifestation. Nectar-bearing glands are usually abortive organs.

We may now say that nature makes a Polemoniaceous flower simply through greater irregularity than usual in the intensity of its growth-waves. When the calycine leaves had been reached the growth-waves were weak or they would have been bracts, and so weak that enough force could not be collected to develop the next cycle at all; a partial recovery resulted only in the " hypogynous disk." The corolla found a fairly steady wave, but all following were unsteady, as shown in the varying lengths of the organs. There should have been five carpels and five lobes to the style, but as in the formation of the "hypogynous disk," the growth-waves were too weak. They sometimes, however, reveal to us their unsteadiness. I have frequently found but two carpels in some Phloxes, while once in *Phlox Drummondii*, I found four!

With the facts as brought out in the making by nature of Polemoniaceous flowers, we can see how with a little better regulation of the growth-energy other genera and species of plants might have been created which now have no existence. The wholly suppressed series might have been a five-cleft or a gamosepalous calyx,—the "hypogynous disk," petals or a monopetalous corolla, and the present corolla, might have been another series of five stamens. With these changes other combinations appear.

We may also see in a study of Polemoniaceous flowers how the varying rhythmic strength influences the whole form of the flower, as well as the development of its individual parts. Dr. Gray says: "the corolla is not always perfectly regular." The tube of the corolla is slightly curved in most species of *Phlox*. Three longer stamens are always on the upper side of the curve, the two smaller are the lower ones. When the seed vessels of *Phlox paniculata* are nearly mature, the calyx is two-lipped, the three upper corresponding to the upper or outward curve of the tube of the corolla. We may say that it is the irregularity in the growth-waves resulting in organs of varying degress of development, that cause irregular flowers. We have regular flowers when the growth-waves of a plant are all of a uniform intensity.

PELLUCID DOTS IN SOME SPECIES OF HYPERICUM.

Many species of *Hypericum* present small black dots on the stems, leaves or other portions of the plant. In *H. corymbosum* Muhl., the stem and lower leaves have these dots profusely scattered over the surface, those on the stem being generally somewhat elongated, after the manner of suber cells as usually seen in the young bark or epidermis of woody plants. Examining the series of leaves in succession up the stem, we find the uppermost leaves nearly destitute of black dots, a few being found chiefly near the margin. Starting again from below upward, we find that as we advance the pellucid dots, wholly absent in the lowermost leaves, begin to be comparatively scarce near the midrib. As they come into existence, the black dots disappear. The pellucid dots increase in numbers with each series of leaves—the black dots seem to give up the ground to the pellucid ones—till, when the much reduced leaves beneath the flower are reached, scarcely any but the pellucid dots are found. They reappear again in numbers on the petals.

It is singular that though there is an evident correspondence in the increase of pellucid dots and the decrease of the black dots, no genetic relationship can be discovered. In no instance was there any evidence of a transmutation of a black dot to a pellucid one.

The dotted structure of other species growing on my ground was therefore examined. These were *Hypericum perforatum*, *H. Buckleyi*, *H. Kalmianum* of our country, and *H. Androsæmum*, *H. caly cinum* and *H. Moserianum* of the old world. *H. perforatum* furnished the most interesting subject for comparison. The lower leaves, given up wholly to black dots in *H. corymbosum*, had only pellucid dots, and in none of these were black dots numerous. Indeed, it was only in the uppermost leaves that a few black dots were found, and these sparingly distributed near the outer edges. On the petals also the black dots are sparingly found.

It is apparent from what has been stated in regard to *H. corymbosum* that the energy productive of the black dots is different in degree from that productive of pellucid dots. We may further conclude that these varying degrees of growth-energy had but little to do with the differentiation of these two species. One species could readily be transformed to the other as each degree of energy was in control.

In *H. Kalmianum* and *H. Buckleyi* pellucid dots are profuse. No trace of black dots could be found. From what has been already noted, they are not to be expected where the pellucid dots abound.

In the European *H. Androsæmum* the whole surface is minutely pellucid-punctate. The mid-rib and veins are also pellucid. At various points along the lines of these veins are, however, small swellings, more or less orbicular, as if they had been originally pellucid dots, and that lines had been thrown out to connect the dots with each other. In *H. calycinum* there are similar connections, and besides a few scattered pellucid dots; but these are not round but pyriform, one end tapering into a narrow tail. If a little more prolonged, we should have just such connections as we find in *H. Androsæmum*. In *H. Moserianum*, we find the surface profusely covered with dots, not very pellucid, apparent though they are when held up to a bright light. Some of these are wholly individualized and isolated, but others have sent out a line as yet unconnected, but the great majority have had the lines connected, and have formed a mass of reticulated veinlets unequalled in any other species I have seen. Turning to *Hyperium prolificum* I find many semi-pellucid dots in the petals, especially near the margin, and some of them elongated, and in a number of cases they have met others and formed an elongated pellucid vein.

I think these pellucid dots are the initial steps taken by the plant in the formation of veinlets and veins. It cannot for an instant be conceived that nature first makes a skeleton leaf and then covers it with parenchymatous tissue. These strengthening ribs must be constructed out of cell-tissue only as the organism needs them. And this construction can only go on under a regularly arranged system. There can be no theoretical reason against the view I have taken of the nature and office of these pellucid dots.

I think little has been written regarding the variable character of these dots. The only author I have found is Bromfield, who was, in a measure, my early patron and preceptor in botanical study. In *Flora Vectensis*, writing of *H. perforatum*, he takes occasion to note the difference in the character of the dots in various species, which, in some, take the form of anastomosing pellucid veins. "I do not find," he concludes, "any notice taken of this character by any author I have consulted." I have seen none since his work appeared in 1856.

HONEY GLANDS OF FLOWERS.

It is impossible to take up any subject connected with the behavior of plants without a thought of the wonderful labors of Darwin in the same line. We owe him warmest gratitude for the direction he has trained us to follow. But some of us believe that the great field of vision he opened up to us is broader than ever he himself suspected, and that many more behaviors of plants are to be seen and interpreted than it was given to him to behold and explain. It is, moreover, clear that the *a priori* line on which he started must naturally bias judgment. It is not in human nature to be free from such bias. Feeling that every act and behavior of a plant must originate in a selfish effort for its own good, the doctrine of natural selection naturally followed. The natural condition of life being that of continual war, every effort of a plant was to secure some advantage in this great struggle. Whatever helped this view could not but be welcomed, even by one who was so unusually fair minded as Darwin. Whatever did not accord with his premises, could not be considered as of much importance. Some of us have departed from the path of our great leader. To us it seems that while selfishness is an undoubted condition of existence, self-sacrifice is equally a natural law. It appears to be the higher development of the original condition—the *raison d' etre* why selfishness exists. Facts which Mr. Darwin would treat lightly, we may be pardoned for desiring to see more clearly elaborated.

The honey glands in flowers have been, in Mr. Darwin's view, so closely related to the encouragement of insect visitors, that their production where they could have little reference to the fertilization of flowers is lightly treated. He refers[1] to an observation of Karr that the bracteas of some orchids secrete nectar, that Fritz Müller has seen a similar behavior in the bract of *Oncidium* in Brazil, and that Mr. Rodgers had seen a similar secretion from the base of the flower-peduncles of *Vanilla*. That he could have seen this frequently in the species of orchids under his own observation is probable. He names *Phaius* as one of the genera in which he examined the flowers for nectar.[2] I am sure I have seen honey glands similarly situated in many orchids, but they are very evident in *Phaius grandifolius*, a common species under cultivation, and probably the one Mr. Darwin had under observation.

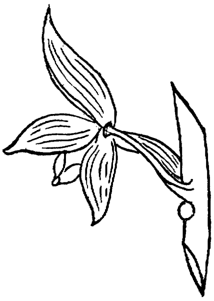

Fig. 2.

I have had before me for a couple of weeks past a Nepalese species not uncommon in gardens, *Cymbidium aloeifolium*, in which the copious supply of nectar from the base of the bract, or rather from the main stem at this point, attracts general attention. It will be of interest to describe the development of the infloresence in detail. The spike has sixteen flower buds on it. The peduncles are at an acute angle with the main stem, and perfectly straight until the bud has reached its full size and is ready to expand. When this stage is reached, the peduncle takes a horizontal position and then makes a twist curving upwardly, and the labellum, which up to this time had formed the upper portion of the perianth, becomes the lower. Many days before this occurrence the nectar commences to ooze from the gland at the base of the bract. Long before the opening of the floral segments the globule has reached its full dimensions of two or three lines in diameter. It soon hardens on exposure to the atmosphere, and has a high degree of viscidity from its earliest appearance. Considerable force must be exerted in expelling it from the tissues of the plant. Mr. Darwin's explanation is that in these cases the excretion is for the sake of getting rid of superfluous matter during the chemical changes which go on in the tissues of plants. But as starch is necessary for storage, and plants generally have no superfluity of the article, why should the plant labor to form that which, in this case, must be the wholly superfluous article of nectar. To get over this difficulty Mr. Darwin had already suggested that nectar was in the earlier ages of plant life always superfluous. That insect life at first had no knowledge of its existence or value, and that on discovering it, insects and flowers became gradually more correlated.

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[1] Fertilization of Orchids, Chap. IX.
[2] Ibid. Chap. V.

So far as we can now see these secretions render the plant no service whatever in the great battle of its life, and this Mr. Darwin frankly owns. To him it is an act of excretion of useless matter. To us who believe that individual life is not wholly for itself, but that every act is of some use in the general economy of nature, the new field opened up is one of extreme interest. Observations in this beautiful field are too limited to warrant any general deduction as to the purpose of these stem-bearing glands. The object of this paper is to draw the attention of those who may have orchidaceous plants to a closer examination of their structure, and to encourage a record of such observations.

VARYING PHYLLOTAXIS IN THE ELM.

Calling, a few years ago, the attention of Dr. John Macfarlane, then assistant botanical instructor in the University of Edinburgh, to a bed of one year old seedlings of *Ulmus Americana* and that many of them had opposite leaves, he further observed that each form had been so characterized from the earliest development of the plant. They were either alternate or opposite from infancy, and had adhered strictly each to its separate character through the whole of the first year's growth. This bed of one year seedlings probably contained ten thousand plants. In different parts of this long bed the numbers of each class varied. In places about one-third were alternate leaved, in others the opposite leaves were much more numerous. For the purposes of this paper it may be assumed that about half were of each class.

In another part of my grounds I had about one thousand two year old plants, transplanted the previous spring when one year old. Interested in learning how long the opposite leaved individuals—for I assumed there must have been many of that character—would continue of that class, I found that all but nine had started on the opposite leaved plan from the commencement of the second season's or last spring's growth. These nine continued to form opposite leaves on all the leading branches, but the secondaries which pushed out from the primaries during the summer, had alternate leaves. One may say that by the third year all trace of the opposite leaved system will disappear.

It has been assumed in phyllotaxy that the underlying law in leaf-arrangement is to provide for the very best exposure to the light of the foliar organs. That leaves must have light, and that leaf-arrangement must surely have some reference to this fact, needs no argument to support it. But it must be obvious to an experienced observer that leaves are very far from being always arranged in the best manner to this end. One need never go far for an illustration that if advantageous exposure to light be all that is involved, the plan could be vastly improved. There has always seemed to me fair ground for believing there must be a deeper undiscovered law, and I have been continually on the watch, without much success, for clues to this deeper mystery.

In the case of these elms, the pairs of leaves are decussate, every other pair being exactly superposed with those beneath. Such an arrangement would not be thought the most favorable for the action of light. On the other hand, the alternate distichous arrangement in these young plants, and measurably in the older ones, causes the leaves in many cases to have the leaf-blade vertical, or, where the nodes are not particularly distant, to have portions of the leaf-blade overlapping another leaf, in effect reducing considerably the available leaf surface.

One point of special interest is that the nine trees which carried over the opposite leaved arrangement through the second year are much weaker in growth than the others. I think I might safely say that superior vigor of growth and the alternate arrangement of the leaves are correlative.

SPECIAL FEATURES IN A STUDY OF CORNUS STOLONIFERA MX.

Some interesting features for biological study are furnished by *Cornus stolonifera* of Michaux, *Cornus alba*, or the " white-berried " Dogwood of many authors.

In northern regions, in southern Canada for instance, it flowers in June. Under cultivation in eastern Pennsylvania, its flowering season is during the first weeks in May. When the axillary buds of the past season push into growth, some of them seem more vigorous than others. Strange to say these vigorous branches are suddenly arrested at the second node, and, instead of a continuous axillary growth, the cymose inflorescence follows. The flowering is over by the first of June, and the white-berried fruit matures by the first of July. But the weaker growths of the past season are not arrested at the secondary node, but make a continuous growth until the end of June, when the axial growth is also arrested and flowers follow as in the earlier instances. These branches are flowering while the earlier ones are maturing seeds. So suddenly is the axial growth force arrested, that the two axillary buds at the base of the axis, which in the condition of inflorescence becomes the common peduncle, have the growth energy communicated to them, and instead of remaining dormant to make side branchlets for next year, start to form branchlets now. Some idea of the intensity of the growth force, and the suddenness with which the energy was diverted laterally, may be inferred from the fact that these axillary buds, so suddenly called into development, will elongate and form a pair of fully grown leaves by the time the blossoms in the cyme have become fully developed. Except that the cyme, equally with its axis, is somewhat weaker than the earlier ones of the season, there is no material difference in the other portions of the inflorescence. The axillary buds at the base of the earlier blooming cymes push into growth with as much vigor as those on the later blooming branches, and some of these terminate in inflorescence, the flowers blooming in August. The facts furnish excellent illustrations of the influence of arrested or accelerated growth force in changing the character and habits of plants.

Another point I have taken occasion frequently to illustrate is one which I believe to be wholly my own:—that leaves by no means always originate at the node from which they seem to spring, but from some indefinite point in the axis below. In this *Cornus* the sudden arrestation of growth which determines the flowering conditions and the lateral divergence of the growth-energy, produces the union of a portion of the petiole with its axial growth resulting in one of the branches of the cyme. In some cases the arrested leaf-blade will form a bristle-like appendage an inch in length, slightly expanding to a lamina at the apex. The united portions of the cyme-branch and petiole can be readily traced downward towards the lower node. The separation of the edges results in the usual square stem at this point. It may be remarked here that one of the distinctions between Cornaceæ and Umbelliferæ is the absence of an involucre. But here we find the tendency to dispute even this character with the Umbelliferous order. The cases sometimes given as illustrating such encroachment are not valid, for the so-called involucre of *Cornus florida* results merely from a second growth of bud scales. This occurs in many families of plants, as, for instance, *Pavia, Carya, Fraxinus* and other genera, especially in some hickories, where the rejuvenated bud scales are sometimes as highly colored as in the Dogwood and go by the name of " Hickory-lilies."

The appressed hair of the leaves and young branches of *Cornus stolonifera* have been referred to as being " straight but fastened in the middle, and thus appear appressed." But many will be noted as having but one arm, which is as much appressed as if it had been fastened at the middle. It is evidently a branched hair. The same general law of sudden arrestation has evidently extended to the hair. The apical growth, suddenly arrested, has driven the energy laterally, resulting in a horizontal growth on either side, as in the formation of the inflorescence.

The tendency to abortion in many of the primordial parts, well noted in *Cornaceæ,* may be illustrated here by some further facts not hitherto specially noted. Though but one seed is produced, two cells are seen in the transverse section of the carpel, and a third has been occasionally observed. With a flower of four sepals, four petals and four stamens, we may reasonably assume the early existence of four carpels. But the stigma is usually described as simple. If, however, we examine it just as the flower is fading, we may note that it is really four-cleft, indicating certainly the four-carpellary character of the ovarium.

Another illustration of the prevalence of abortion is in the small number of fruits which come to perfection in comparison with those which fail. The flowers seem all perfect. It is safe to say that in regard to seed-bearing nearly every flower is potential in all its parts. Pollen and stigma are fully functionable. Indeed there is scarcely a flower but is fertilized and seems to make a fair start toward maturity. But the majority fall by the way. Though the average number of flowers on a well-developed cyme may be one hundred and fifty, it is rare to find a dozen fruits mature. Often there are none. I have not been able to satisfy myself that the anthers discharge their pollen on the stigma before the expansion of the corolla, thus insuring self-fertilization beyond all chances, as I have shown to be the case in many other instances. I have not found pollen in an unexpanded flower, nor any anther that was not covered with pollen in an expanded flower. The discharge of pollen and expansion of the petals is probably simultaneous. The stamens are longer than the style, and one may say, almost with certainty, that the flowers receive only their own pollen. Facts might be adduced in support of the proposition that it was an instance of sterility from the lack of pollen from other flowers, but the weight of evidence will, I think, favor the conclusion that its failure is from abortion. In other words, the sudden arrestation of the growth force disperses the energy into other channels, with sterility as the result.

The winter color of the past season's branches has attracted attention. The rich reddish-brown has given it the popular name of "Red-twigged Dogwood." The manner in which this red tinge is produced is plainly discernible. I have shown in other papers that color in vegetation is mainly an incident in the struggle for life of the various parts of a plant. If we cut off a branch from a tree, it dies without a struggle. If a frost come early in the fall, the leaves blacken and take on no bright tinge. But if we only partly detach a branch from the tree, or if, in the autumn, there is a struggle with physical forces before death finally assumes control, the leaves color. All this I have fully elaborated elsewhere. The same course is evident here. On the young bark, a few white dots appear. These are incipient cork cells. For a while they are inactive. As soon as they begin to develop, the epidermis turns white, and at once a light pink ring encircles the vesicle. When rupture takes place, a pink line extends upward and downward, and from this pink line the reddish tinge becomes gradually diffused. In a short time the whole of the epidermis, both of the young branch and of the petioles, has a reddish-purple tinge. No one who will make a few observations during midsummer will fail to be convinced that the formation of cork cells is a destructive agency, and yet an agency of comparatively slow action; that color is the result of this protracted struggle for life; and that the peculiar action of the cork cells, varying as it does in almost all plants, each species having its own peculiar method of developing cork cells, fully accounts for the red color of the annual growths in *Cornus stolonifera.*

FOLIAL ORIGIN OF CAULINE STRUCTURES.

The endeavor to conceive axial and foliar organs as morphologically distinct, leads to difficulties easily surmounted by the conception that every part of a plant is but modified leaf-blade. In striving to regard stem and leaf as essentially distinct entities, we become wholly lost in studying the genesis of the tendril, and we are compelled to say of them they " may be axial or they may not. This may ordinarily be determined by position. Any direct continuation of stem or branch must be of an axial nature, that is, of the nature of stem ; and the same is true of whatever primarily develops in the axis of a leaf. Conversely, whatever subtends a lateral axis or branch may be taken for a leaf or foliar production being in the place of such."[3] But the difficulty of carrying this idea along to a consistent conclusion becomes apparent at p. 118 of the work cited, where the tendrils of Cucurbitaceæ are pronounced "peculiar and ambiguous, on account of their lateral and extra-axillary position and the manner in which the compound ones develop their branches."

That a leaf and stem must be morphologically the same seems proven by the well-known fact that leaves often develop into stems. The author has seen buds at the ends of the leaves of the Japan Umbrella Pine, *Sciadopitys*, and a large number of species of plants are raised by florists from leaves of which *Begonia* is a familiar example. The section of an articulated stem of *Opuntia* is technically stem; yet the flower which, morphologically stated, is but a "bundle of leaves," often shows that it is simply a whole section metamorphosed. We call the deciduous portions of *Taxodium distichum* "leafy branchlets" simply because the least vigorous ones perish. If all were to fall, they would be regarded as pinnate leaves, and it is barely necessary to refer to the fact that numerous plants bear flowers on the leaves, as a tree of the Jujube, *Ziziphus vulgaris,* before the author as he writes, amply illustrates.

By adopting the hypothesis that a tendril is always cauline, we shall have to admit that stem is of folial origin in a study of Bignoniaceous plants. A number of species have trifoliate leaves when the branches are comparatively weak, but when they have gathered strength the terminal leaflets develop into a strong tendril. For instance in *Bignonia venusta* "the leaves are opposite; lower ones ternate and without tendrils, upper ones conjugate or pinnate with one pair of leaflets, and furnished with tendrils." The quotation is from the *Botanical Magazine*, Vol. 46, p. 249, so that those who have not the plant at hand, may see the different forms of leaves in the plate. The one leaf has become a petiole, and is again trifid at the apex—these branchlets again becoming leaflets in other species. Whoever has had the opportunity to study some of the strong growing Bignoniaceæ of Central America, must be fully impressed with the woody character of these tendrils. They are as truly ligneous as are those of Vitaceæ, which are conceded to be of axial origin. When, therefore, we find a plant with a leaf normally trifoliate, transform the terminal leaflet into a permanent woody tendril, it is difficult to understand the necessity for the "abnormal and exceptional" view of these cases. If we say leaf-blade is the foundation of all cauline structure, we are not surprised when a leaflet of *Bignonia* becomes of the nature of stem, and we are not left to mere position on the axis before we can determine the origin of what is actually the same thing.

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[3] Gray's Structural Botany, 1879, p. 54.

POLARITY IN THE LEAVES OF THE COMPASS AND OTHER PLANTS.

Those who try to live and learn are often suprised to find some conclusion, which in earlier years we thought unassailable, suddenly shattered by the logic of facts. In my own case it is not unusual to note that beliefs which in a limited sphere seemed founded on a rock, were wholly washed out when opposed by a more general application. Of this class is my former faith in the polarity of the root-leaves of the Compass Plant, *Silphium laciniatum.*

It is just as true to-day as it ever was that the root-leaves stand erect instead of curving more or less toward the horizon as the root leaves of most herbaceous plants do, and that the edges of these leaves are in a more or less north and south line. The faces of the leaves are of much the same construction, and are presented to the east and west respectively. It seems very reasonable to suppose that the leaves are polar because of the similarity in the structure of what would usually be the upper and lower surfaces; that because both surfaces are struggling to get to the full light, and having equal power neither can win, and hence the edges must of necessity be in the northerly and southerly directions. This is the accepted explanation of this supposed polar arrangement.

The first blow to my faith in this doctrine was the observation that though the stem leaves have an equal arrangement of stomata on either surface, just as the root-leaves have, they make no attempt at "polarity." Further than this many Australian and Cape of Good Hope plants have vertical leaves, with an equal distribution of stomata on either surface, but there is no polar direction specially to the edges of the leaves. In our own country the leaves of *Quercus Catesbœi* are all vertical the first year from the acorn, but their edges are directed to any point of the compass. It is evident that we must yet regard the question as to the cause of the northern and southern direction of the leaf-edges in the Compass Plant as an open one.

If we now look over the whole field of Compositæ, it will surprise us to note how many of this order have the whole or portions of the leaf-blade vertical. *Liatris, Lactuca, Chrysopsis, Tanacetum, Cirsium, Centaurea, Boltonia, Silphium, Mulgedium* and *Sonchus* will furnish numerous illustrations. No one can fail to see that this results from a continuation of the spiral growth into the leaf-blade. In many cases there are two spiral twists in a single leaf-blade; the leaves on the flowering stems of these genera are on the $\frac{2}{5}$ plan. The epiderm is composed of five leaf bases or dilated petioles ascending spirally and lapping over each other, and this spiral motion is continued to the free portions which we designate as the proper leaves. This spiral motion is greater in some species than in others. In *Liatris* it is particularly marked. Not only does the twist become so severe as to turn the leaf-blades strongly on their edges, but extends to parts of the inflorescence. Even the style branches are twisted around each other, and the styles of many different florets will coil into a tangled mass. Many other orders besides Compositæ have the tendency to carry a portion of the leaves vertical, wholly through this richness of spiral development. Onagraceæ furnish abundant illustrations; and the coiling of the leaves of the garden *Narcissus* must be familiar to many. We cannot consider the condition of such leaves to have any more physiological importance than if they had presented a perfectly horizontal plane.

What then is the explanation of the generally north and south line of the leaves of the Compass Plant, admitting, as we must, that the vertical position is merely the result of vigorous spiral development?

We may get help from the behavior of plants in analogous cases. I observed many years ago, and placed the fact on record in the *Proceedings* of the Academy, that in going across the prairies of Illinois, the earlier flowers of *Helianthus mollis* faced the southeast. This I have confirmed by plants in my garden. Not only is it true of this, but of other species of *Helianthus.* I could travel across the open prairie as well by these flowers as by the leaves of the Compass Plant. The opening flowers of many other species of plants are usually in nearly one given direction. *Digitalis media* of the Old World, has the opening flowers mostly in a direction facing south. I have never found any plant, growing in a fully open place, present any tendency to open its flowers in a northwardly direction.

Now let us for a moment pass to the anthesis of some very early spring flowers. We will take the Goat Willow, *Salix caprea* and the Chinese *Magnolia Yulan.* If the male catkin of this willow be in a position to catch the early morning sun, the catkin has a curve westwardly; if the aspect is such that it cannot get the sunlight till midday, the curve of the apex of the catkin is northwardly. It is exactly the same with the flowers of *Magnolia.* A very slight warmth from the sun at that early stage of the season excites growth, and the development of the flower being greater on the warmed side causes the apex to seem to curve in the opposite direction. We may conclude that some such law prevails in the opening of the sunflowers and others. All plants have varying phases of rhythmic rest and growth; many Compositæ start their daily growth, as I have shown in various papers, soon after sun-rise. A little extra warmth at this time, would throw the greater growth in the easterly direction. A flower, the period of rest of which ceased at noon time, would have its greatest development encouraged in the westerly line.

Just how such considerations as these would affect the root-leaves of the Compass Plant is not clear. But if we imagine, as we may from the facts detailed, that growth in the spring is favored in one point more than others, and that there is a favored resting as well as a growing point, we have a good clue to a sound explanation. I feel assured that the final solution of the peculiar position of the leaves in this plant must be sought for in this direction, and not in any peculiar physiological necessity on the part of the plant itself.

HYBRIDS IN NATURE.

Our gardens abound with hybrid plants. Although the gardener's skill originates them, there seems little reason why they should not occur in nature. The plant desired to produce seed has its flower opened by the hybridizer before it naturally expands; the anthers are removed before the pollen-sacs are ruptured, insects are excluded, and the next day probably, when the stigma is receptive, the foreign pollen is applied. In this way hybrids are secured. From the ease with which hybrids are produced in this way arises the belief that hybridism in nature must be of frequent occurrence.

It is a matter of grave importance that we decide how far this belief is correct. Up to a period not remote, it was a belief that what we know as a species was always a species from the earliest epoch. When a distinct form came under observation that seemed not to have existed from the beginning, it was regarded as a hybrid. It would be accepted as a species, though deemed of hybrid origin. Thus Linnæan nomenclature abounds in "*hybrida*" as a specific denomination. If it can be shown that these are not hybrids, but have been evolved from other species under some regular law of development, the importance of the question becomes apparent.

We now accept the doctrine of evolution as beyond discussion. Species do follow from other species as the world advances; but the old idea is still so prevalent, that many botanists who accept the facts of evolution in a general sense are very apt to regard any unusual departure as a case of hybridism. Our modern literature abounds in such instances. Supposed hybrids are being continually described as actual hybrids on no other ground than that they possess characters common to others already described.

If nature intended hybridity to be one of her handmaidens in the production of new forms, she has strikingly failed. Let us take the oak as an illustration. When the male flowers are at a certain stage, a slight jarring of a branch will cause the pollen to float away in little clouds discernible to the watchful eye. One may readily conceive what an enormous quantity of pollen must be carried from one tree to another by every sudden breeze. In our woods there are rarely less than two or three species in company. Not infrequently there are more, and these are usually blossoming all at one time. Hypothetically, one may argue that these gregarious species must receive one another's pollen, must cross-fertilize, must result in a hybrid progeny in which every separate characteristic will be irretrievably lost. But the careful student of nature knows that this is not so. The seed collector goes into a wood which may contain White Oak, Black Oak, Scarlet Oak, Red Oak, Chestnut Oak, Swamp White Oak, Post Oak, Black-Jack Oak, Scrub Oak, as he may do along the Wissahickon, gather the acorns of each species under its particular representative, and plant them with the absolute certainty that they will be true to their several parentages. This could not be if the hypothetical proposition cited, of free inter-pollination, were an actual fact.

How, then, are we to account for the striking deviations from typical forms which we occasionally see? I have long believed that form is the result of various degrees of rhythmic growth. It is the mechanical result of varying degrees of energy. These results may be noted on a single tree. On the weaker branches of a white oak the leaves will be comparatively entire; on the stronger shoots, where growth-energy is rampant, the leaves will be deeply lobed. In mulberries these differences must be well known. The leaves on branches full of growth-vigor are lobed, but when this energy is somewhat spent, wholly entire leaves follow. Surely these facts must have come within the range of common observation.

But varying degrees of rhythmic growth may not always result in lobed leaves in its aspects of vigorous growth, or of entire leaves in its weaker ones, because other factors interfere. We may not know just what these incidental forces are, though we may feel sure they exist. For instance, on the common red cedar we may note two distinct forms of foliage: on the weaker, half-starved branches the leaves are like needles and resemble those of the common juniper, but on the more vigorous branches there are seemingly no leaves at all! We have to say "seemingly," for indeed there are really leaves, as really so as on the weaker ones, but the peculiar growth-energy of these more vigorous branches causes them to become connate with the stems. On a branch a year or two old, we can easily separate these connate leaves from the true bark formed beneath.

But that there is no necessity for bringing in hybridity to account for the occasional aberrations from the normal form we meet with is well known to those nurserymen whose business it is to raise trees in great quantities. There are just as many and just as striking variations among genera consisting of a single species, or of species wholly isolated from other species of the genus, as where there are several. The European Oak, Ash, Linden, Beech and many others furnish illustrations. The English Oak, *Quercus Robur* especially, will furnish scores of variations that have been selected from the seed-beds of nurserymen, and given distinctive names. Many of these differ from each other by characters quite as striking as those which distinguish American Oaks from each other; but we know they are not hybrids because there was no other species with which they could intercross, and they are not regarded as species because of their derivation from *Quercus Robur.* This would not be a true test of specific rank. It still savors of the old doctrine of the special formation of species which we know is not true. With our modern experiences we may expect occasional wanderings from a general character as a result of an unusual expenditure of force. Usually these displays of energy are not able to maintain themselves. Seedlings fall back to the habits of their ancestors. If, however, they should be able to maintain themselves, they are entitled to rank as species. They are species and nothing else.

Seeing, as we must, that all this is so, and must be, why should we refer to hybridity to account for individual changes, especially as the warmest advocates of natural hybridity rarely get beyond "supposition" in any case.

ORIGIN AND NATURE OF GLANDS IN PLANTS.

When treating of glands, authors point out their secretory and excretory functions, and describe their structure and general characteristics, concluding with the bare statement that "besides these there are others which seem to have no office in the general economy of the plant." It has always seemed to me that this class of glandular bodies deserved closer investigation, and in 1869 I contributed a paper to the meeting of the American Association for the Advancement of Science, at Salem, "On the Glands of *Cassia* and *Acacia*,"[4] indicating that in these and some other plants the "glands" could not be otherwise than abortive branches. Though the paper excited no discussion, it is fair to say the proposition did not seem to meet the approval of the eminent botanists attending that meeting; but, so far as I know, no further attempt has been made to investigate and explain the nature of these so-called glands. It has always seemed to me a fair inference that if they are but arrested or abortive branches, we might reasonably expect to find occasional instances of an advance from their depauperized state.

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[4] Proceedings of Am. Ass. Ad. Science, Vol. XVIII, p. 260.

This season I have had a piece of good fortune; it is interesting enough to give the discovery in detail:—

On my grounds I have a mass of *Cassia Marilandica*, probably fifty plants, covering an area of about sixteen square feet. The leaves are, normally, abruptly pinnate, having a number of pairs of opposite pinnæ with a gland terminating the common petiole. The base of the petiole is tumid, and usually the " gland " arises on the upper surface of the petiole, just above this tumid part. This, however, is not constant. In some cases I found the " gland " a quarter of an inch, a half inch, an inch, and, in a few cases, as much as two inches away. In a few instances they were found with fully-developed pinnæ on either side of them, presenting precisely the appearance of the terminal pair at the apex of the common petiole. As the terminal one has developed with a perfect leaf in some instances, as already noted, it must be assumed that these at the base of the leaf could, under equally favorable conditions, do the same; in their nature they are identical.

Besides this positive proof, the larger basal glands will occasionally furnish strong presumptive proof. These bodies vary much in form, sometimes narrow cylindrical, sometimes nearly globular, at other times are found some remarkably vigorous and ovoid. At the apex may often be seen a pair of small glands in the position that two opposite leaflets would occupy, with a very small gland between, just as we find at the apex of the common petiole. No one comparing such a gland with the apex of a leaf could come to any other conclusion than that the two were normally the same, except that the two leaflets had not developed, leaving only a pair of glands to represent them.

But, it might be suggested, this would only show that the gland was an undeveloped compound leaf; the proof that it was an undeveloped branch would be still desirable. Here we have to fall back on advanced knowledge in vegetable morphology. Most botanists are now prepared to believe that not only are the organs of flowers to be conceived of as modified leaves, but the stem itself comes under the same conception. The cell is the primary plant. Whatever form the plant has to take ultimately, is governed by laws operating in this primary cell. The next step in the conception is the union of cells so as to form leaf-blade, and the next the coiling of leaf-blade to form stem, the upper portion of each coil finally becoming the leaf. There is abundant evidence, not necessary to be reproduced here, that this theoretical conception of the formation of stem by the mere coiling of leaf-blade is legitimate, and we are left only to account for the formation of buds, which are eventually to become branchlets.

The origin of buds is readily accounted for by the operation of rhythmic growth. Growth is not continuous, but progresses by a system of successive waves. The node is at once the resting place of the old wave and the departing point of the new. The axial bud is really the termination of the old growth wave. In many cases during the resting period, it starts rapidly on with the new growth-wave, assuming its original position as the leading shoot, when what might have been the leader from a lower portion of the system of arrested nodes forming the bud, is pushed aside to become what is known as an extra axillary branch. This conception I cannot claim as the belief of " most botanists," as it is essentially my own, supported by facts detailed in many papers, which it is not necessary to reproduce here.

Recalling the evidence directly showing that the " gland " must be a branch arrested in development, and carrying in our minds the theoretical conception of the origin of the stem and buds, we can readily understand how an arrested branch has been enveloped by a coiling leaf-blade or set of leaf-blades on the petioles until all that is left is consolidated apex which we now call a gland, and how glands of this character which appear higher up on the leaf stalks are still modifications of the same character, the whole compound leaf itself being of precisely the same character in its original structure as what we come ultimately to know as stem and branches.

In my paper above cited I introduced the bud system of the Honey Locust, *Gleditschia,* in confirmation of the position assumed in regard to *Cassia.* Here again it furnishes evidence of the soundness of the conclusions reached in this paper. The leaves are abruptly pinnate as in *Cassia,* but the apex of the common petiole or rachis does not appear as a gland so much as it does an undeveloped bud. Occasionally, however, it does develop, and the leaf then has a single developed pinnated leaflet instead of the usual abruptly terminating pair. In other instances one of the opposite pair is abortive, in which case the one developed assumes the role of a terminal leader, pushing aside and making axillary to itself the usual terminal bud or gland. This latter instance strongly sustains my point that when a subordinate bud gets the opportunity to push vigorously, the whole system of buds above, arrested in its growth by some varying of rhythmic action, is pushed aside to the "extra-axillary" position.

The character of rhythmic growth-waves has not yet received much attention from vegetable biologists. We know that in the growth of some plants the wave progression seems regular and continuous, and then the form of inflorescence we term centrifugal results; at other times the rest seems but partial and is resumed retro-actively, presenting to us the centripetal condition. Though ignorant of the manner in which these varying phases of rhythmic growth is brought about, there is no question about the fact. The production of the supra-axillary bud into a spine in *Gleditschia* is, therefore, easily explainable. Assuming, as has been done in this paper, that an axillary bud is the termination of a growth-wave, and that the growth which has succeeded in establishing itself as leader pushed the bud—the arrested terminal branch—aside; a lower bud on the arrested branch in the more active line of the subsiding growth-wave, would push into activity sufficiently to become a spine, while all the upper portion of the terminal axis remains for a bud, or, perhaps, a mere "gland." In this case the spine represents a centrifugal growth. But when what appears to us to be a lower bud, but which under the conception here adopted would be an upper one, first pushes, we may regard it as an illustration of centrifugal growth in the bud state.

I may be pardoned for observing with some pride, while making a contribution to the knowledge of other hitherto obscure phases in plant life, that the deductions made in my Salem paper of twenty-four years ago have been fully confirmed.

NUTRITION AS AFFECTING THE FORMS OF PLANTS AND THEIR FLORAL ORGANS.

It is remarkable that while American students of plant life are leaning more and more toward the belief that variation in plant structure is due to varying degrees of life energy, European botanists seem more inclined to search for external conditions as the great factor in these modifications. That internal energy must be influenced to some extent by external conditions is manifest to every observer, but changes brought about by these outside influences are rarely permanent. Efforts have been made to show that acquired characters have become hereditary; but in most of the instances adduced, it has not been difficult to prove that the changes, wholly credited to outside conditions, simply hastened the internal action which could have been taken wholly independently of these conditions.

It has fallen to me to show, first in a paper published in the *Proceedings* of the American Association for the Advancement of Science, 1868, and subsequently in other papers, that form is the result of varying degrees of life-energy, and that the degree of energy is due to the ability of the whole plant or portions of a plant, to elaborate nutritive material. This being true, external conditions may have to do with the supply or the character of plant nutrition, but it is the constitutional power of a plant, or any portion of a plant, to avail itself of nutrition that determines the resulting life-energy.

The present paper has been suggested by reading an excellent memoir in the Linnean Society's *Journal*, Botany, Vol. XXXI.[5] It starts out with this proposition: "The capability of varying is admittedly a general property of all living organisms, but how variation is affected by forces other than natural selection we know but little."

I have shown in my address before the American Association at its Montreal meeting,[6] that variation is an essential condition in the general order of things, and that the reasons for the innumerable variations in the forms of leaves and flowers must be sought for in the necessity of variety for variety's sake, and not in the mere accident of external conditions. In other words the internal energies of the plant, varying as they do in results from their varying degrees, would produce innumerable variations, *although the external conditions were exactly the same all over the world.*

In this paper and another correlated in the *Journal of Botany* for January, 1896, Mr. Burkill records the results of patient and careful studies of variations in the number of stamens and carpels of several species of plants in which these variations have been conspicuously known. But he starts with supposition already referred to, that different external conditions govern the results. He, therefore, divides his examination of the common chickweed into classes accordingly: plants from hayfields, damp locality, plants from a garden rather shaded, plants from a warm cinder bed, plants under the shade of evergreens, and so on through six others. Though no less than 7,951 flowers in thirteen widely separated genera were examined—5,700 being of the common chickweed—the author states that the results are "insufficient to establish any fact other than that plants do vary," but supports " the main contention of this paper that *the position of the flower on the axis affects the sexual organs if they vary*." This is simply my own doctrine on the origin of sexes in flowers[7] given at the Salem meeting of the American Association, that the position of the flower on the axis in relation to the supply of nutrition, or the ability of the organs to avail themselves of the nutrition provided, decides the character of the sexual organs.

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[5] On some Variations in the number of Stamens and Carpels, by I. H. Burkill.

[6] "Variation in Nature," Vol. for 1871, p. 392.

The laborious and valuable observations in these papers result in showing a correlation in appearance and disappearance of stamens and carpels in their variable species. When the stamens were increased in numbers there would be an increase in carpels and *vice versa*. A change in one organ resulted definitely in a change in others. This I have observed in the common *Dahlia* in gardens. The ray florets in the normal " single " form are pistillate, the tubular disk florets are hermaphrodite. In what are termed " double " Dahlias—that is, when the florets become all ray-like or ligulate—the purely female condition follows with the change from tubular to ligulate. I believe this is true in most cases where the tubulifloral section of Compositæ assume liguliforal conditions, as it is a fact well worth noting in connection with this whole subject that while *Dahlia, Helianthus, Bellis, Chrysanthemum* and numerous other species with normally tubulifloral florets will advance occasionally to the liguliforal or so-called double condition of the florists, I can recall no instance of one normally of the latter class that assumed the tubulifloral or " single " condition.

The observations of Mr. Burkill, as well as my own long-recorded observations, show that the variations in the various organs of plants cannot be accounted for under the chapter of accidents commonly known as natural selection or conditions of environment; but are arranged under a definite plan, governed by the degree and condition of vital energy, and that this energy itself is dependent on the supply of nutrition, including the life-power of the cells interested to avail themselves of it.

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[7] See Proc. Am. Assoc., 1869; also American Naturalist, 1869, p. 260.

SOME NEGLECTED STUDIES.

Botanical pleasures need not end with the first frost. Buds and branches furnish an endless variation, and are capable of affording characters quite as reliable as, and, in many cases, more reliable than, those offered us by leaves, flowers or fruit. For morphological or physiological study, a knowledge of the characters presented by buds and branches is invaluable.

A few days ago I came across a very thrifty Pin Oak, about twenty-five years old. Along the smooth, clean trunk, during the past season, a number of weak shoots had grown. I believe we cannot tell how an apical cell, which seems to be required before the growth of the branch can be started, can be formed out of an ordinary wood cell and be able to push its way through a layer of bark a quarter of a century old so as to produce the growth of twigs in question. There is an original field here for study as well as a theme for admiration. Perhaps my own discovery, published in the *Proceedings* of the Academy many years ago, on the nature of warts or excrescences on the trunks of trees, such as we very often see on the Weeping Willow, the Garden Cherry and other trees, may furnish an explanation. It is briefly this: New wood is formed by germination from original wood cells. These are added laterally during the growing season. The last series of cells born of the mother cells at the end of the season become liber cells, and give the new layer of bark for the coming season. But an occasional cell does not change. It continues to be a wood cell, though surrounded by others that have been transformed to bark. It does not separate from its woody matrix, but goes on forming its own additional wood cells, and in the autumn, its layers of bark cells in a sort of colony of its own. These are developed in every direction round the circular matrix, and the excrescence naturally forms a circle. An excrescence, sawn asunder, exhibits the annual growths of wood and the annual deposit of bark, just as the mother trunk does. I have never observed the excrescences make branches.

The manner in which buds are formed and protected at different stages of their growth affords endless pleasure. In *Liriodendron* the stipule encloses the younger growth, and, opening the bud, we find the leaf blade has its apex fast in the axis between the branch and the petiole. No one can doubt that the truncate leaf results from its early casting in such a mold. In *Magnolia* we find the same protection from the stipule, but the petiole is not bent.

The young leaves are folded longitudinally. We can see some of the processes by which nature makes *Liriodendron* differ from *Magnolia,* but what induces the curving of the petiole in one instance and the straightening in another we have yet to learn.

Though no reference is made of the fact in descriptive botany, the manner in which the base of the petiole folds over the young bud is distinctive of the genus *Rhus,* or at least, of many species, for I have not examined all. The folding is so nearly complete that no axillary bud is visible. In the winter, after the leaves have fallen, we see by the cicatrices that it was a fold of the petiole and not an absolute over-growth. The cicatrix is precisely like that formed by the fallen leaf of the Horse Chestnut, and adds another suspicion to a list already by no means brief that there is a closer relation between the natural orders Sapindaceæ and Anacardiaceæ than systematists generally believe. Other species of trees, notably the Plane and Yellow Wood, have similar embracing petioles. There seems no physiological advantage in these cases. The young bud must have some protection in infancy, and variety seems an essential part of the order of things. All we can say is that this form of protector is as good as any other. The internal arrangement of the bud in *Rhus* is interesting: Two bud scales meet face to face, and closely press their edges together. The interior is a cavity, but densely filled with short, soft hair.

A Sapindaceous plant allied to the Horse Chestnut, *Kolreuteria paniculata,* a small tree from Japan, has branches interesting from the fact that the petiole disarticulates at a little distance above the base of the petiole, leaving lacunose cicatrices, and giving the branch a singular knobby and rough appearance. Here again the teleologist will be at a loss, and seeing that it is no disadvantage, we can only say that it gives a pleasant variation to the run of life.

I might offer many illustrations, but enough has probably been said to show how much of interest the winter season may afford.